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AUTHOR Bender, M. Lionel
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ABSTRACT

In this paper, the multilateral comparison method of classifying languages is described and analyzed. It is suggested that while it is espoused as a simple and reasonable approach to language classification, the method has serious flaws. "Multilateral" or "mass" comparison (MC) is not a method of genetic language classification, but a pre-theoretical step preceding comparative/historical reconstruction. MC classification of languages into different families is based on discovering words in different languages that are similar in sound and meaning. Refutation of the method is based largely on statistical analysis of the probabilities of arriving at correct answers, using a test found reliable in several previous studies. In this case it is applied to a genetic classification of African languages. Criticisms focus on errors, uneven documentation, and data not supporting the findings. Several reasons are offered for the good results obtained despite flawed data and analysis, with examples provided from East Sudanic and Nilo-Saharan analyses. Contains 17 references. (MSE)

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TESTING MULTILATERAL COMPARISONS IN AFRICA

M. Lionel Bender
Foreign Langs. and Lits.
SIUC-Carbondale

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1. The general problem

1.1 Genetic classification for the masses

Over-generalizing a bit, it seems to me that there has been a polarization in the field of genetic classification methodology in recent years. The extreme ends of the scale are:

The "botanical approach": one must reconstruct "from the bottom up" through all sub-families to the phylum level.

An obvious problem with this approach is this: how does one know in advance what the detailed structure is? It is obvious that work must proceed "from the top down and from the bottom up" simultaneously- e.g., you cannot do Italic correctly without knowing something about Germanic and Indo-European.

Other extreme statements are also made by some "traditionalists", e.g. only morphological innovations are legitimate evidence for genetic classification, one must not only find regular correspondences, but also account for all exceptions to regular correspondences (W. P. Lehmann- source not at hand). Such dogmatic conditions set requirements which have been only ideals even for the most-studied fields such as comparative Indo-European.

The "fishbowl approach": there are "global etymologies", relating all world languages, transparent by inspection to all but Comparative/Historical (C/H) linguists (the parallel with the Emperor's New Clothes, visible to all but fools, is striking).

Language classification is really quite easy and can be done by anyone! Ruhlen 1994: viii, quotes Greenberg as follows: "to really screw up classification you almost have to have a Ph.D. in historical linguistics. Ordinary folks, with no training, inevitably arrive at the correct solution". This is made possible by "the method of Multilateral Comparison".

1.2 The cloudy fishbowl

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But "Multilateral Comparison" (MLC) aka "Mass comparison" is not really a method of doing genetic language classification: it is a pre-theoretical step preceding Comparative/Historical (C/H) Reconstruction, which is the real method. This is admitted even by the practitioners of MLC (v. Ruhlen 1994: 130 and Greenberg 1987: 27), but they state that what they are doing *is* classification preceding the comparative method, which presumably does something else. One wonders "why bother with the C/H method?" if classification without it is so easy (and in fact, MLC practitioners do not bother with it). But one does not usually elevate pre-theoretical inspection of the data to the status of method. For example, do syntacticians set forth their introspective musings as method be-

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fore undertaking the actual syntactic analysis in some theoretical framework?

An explanation of how MLC works, extracted from Ruhlen's book for the layperson (1994: 8-9) is as follows: "The classification of languages into different families is based on discovering words [sic] in different languages which are similar in sound *and* meaning... Throughout this book you will be shown tables that list words in different languages, and you will be asked, on that basis, to classify the languages into families...Your task is to classify these languages into language families simply on the basis of perceived similarities...since the meaning of all the forms is the same, you need concern yourself only with deciding which forms are similar in their constituent *sounds*".

In the book at hand, meanings are said not to differ within the lists given, but in practice in MLC work (such as Greenberg 1963), similarities in both sound and meaning are judged by the investigator, amateur or otherwise.

A classical statement of why MLC is presumed to work is given by Greenberg in an early paper (1953: 271-2) and repeated in his 1963 (3). If the probability of acceptable similarity by chance between languages A and B on a given item is p, then the probability that a third language C has an acceptable similar item by chance is p^2, of a fourth language D having a similar item by chance is p^3, etc., to p^{n-1} for n languages. The probability rapidly approaches zero, e.g. at 5% chance similarity, for four languages, $p^3 = .000125$ (1 in 8000).

But there is an overwhelming fallacy in applying this reasoning. The argument as stated applies to the case in which a similarity has already been noted in two or more languages. But the situation of interest is actually one in which one begins with a set of languages, say 12 of them, and then looks at a particular item on the word-list, e.g. 'hand'. Starting with language 1 on the list, one then looks at language 2: the probability that a match will *not* be found is 19/20 or .95. The probability that a match is not found with L1 vs. L2 or L1 vs. L3 (if all the L's are independent of each other) is then $.95^2$ or .90 (rounding off). This continues till L12 has been reached and the probability that no match is found in any of the 11 cases is $.95^{11}$ or about .57. [1]

In other words, by just looking at L1 vs. L2...L12, there is already a probability of about .43 (=1-.57) that at least one match is found even if all L's are independent of each other. But there are still 55 more ways to choose pairs out of the 12 languages, starting with L2 vs. L3...L12, then L3 vs. L4...L12, etc.

If a pair is found, then there are 10 ways it can be extended to a triple by looking at the remaining languages. In fact, while p^n is *decreasing*, the number of ways one can find 2, 3, 4, etc. sets of languages out of a given number (e.g. 12) *increases* to a maximum at half the number (in this case 6): there are 66 ways to choose 2 (or 10) out of 12, 220 ways to choose 3 (or 9) out of 12, 495 ways to choose 4 (or 8) out of 12, 792 ways to choose 5 (or 7)

out of 12, 924 ways to choose 6 out of 12. The mathematics is much more complicated than the simplistic "powers" argument implies and it is clear that the chance of getting an n-ary comparison is much larger than the simple p^n because there are so many ways of choosing pairs, triplets, etc.

Two practical objections also arise:

(i) rarely are more than two or three languages involved in the comparisons (see in 4 below for examples from Greenberg 1963);

(ii) MLC practitioners allow themselves wide latitude, unconstrained in any principled way, as to what is acceptable as "similar" in both sound and meaning; this greatly multiplies the chances of accepted matches (i.e. it makes *p* much larger than 5% or such values- Greenberg 1953 used 8% as an illustration- in fact, it can easily approach 1 -certainty- in such unconstrained cases). The principled phonological constraints are provided by the comparative method. Constraining semantics is an unsolved and perhaps unsolvable problem, but there are practical means such as accepting only semantic shifts documented in the family or area or a fixed small number of synonyms.

It would be difficult to set up 12 items in which no two look "similar" by the standards used by the MLC practitioners. Let us look at the word for 'hand' in the 12 representative N-S languages I use in 2.3 below:

A: Gao *ka(m)ba*	B: Kanuri *musko*	C: Aiki *kara*
D: For *-oŋa*	*E: PE.S. *asi*	*F: PC.S. *sili*
G: Berta *θaba*	H: Kunama *kona*	I: Twampa *meɗ*
J: Sai *ela*	K: Ik *kʷɛta*	L: Krongo *niiso*

The sets A, D, H and C, J are likely choices by the MLC method, especially bearing "movable k" in mind (the phenomenon noted by Greenberg [1963: 116, 132] that in N-S nominals sometimes appear with or without *k-*, from language to language or even within the same language). More adventurous sorts might include *E and *F and even perhaps L as a set; even A and G or B and L cannot be ruled out as possibilities. The "MLC Method" does indeed make it easy even for the lay person to relate languages!

In my reconstruction work, the C, *E, G, H, and K items above occur singly in five different isoglosses, while *F and J occur in my #191 (along with Fp *(h)eli* and I Opo 'elbow' *sil-*). Thus, MLC would lead to completely wrong results in this instance.

Refutation of MLC must be statistical because the question is a statistical one: it is one of probabilities, not possibilities (as stated by Franz Rottland at the Prague Round Table on Lexical Diffusion in Sub-Saharan Africa, August, 1993). The question is "How much is enough?" either in terms of numbers of positive instances or in terms of "quality" of examples. Quality itself must be made objective, i.e. numerical, in some way if we are to pass beyond mere subjective judgments. It is easy to accept genetic relatedness of English and German or Italian and Spanish by inspection

(recognizing that plenty of high-quality comparisons can be produced), but the interesting cases are precisely those for which such easy judgments are not possible.

The basic question is: what statistical measure can be applied to state that languages A and B are likely to be genetically related at a given confidence level? The test developed by Donald Ringe (1992, 1993) provides one answer to this question. *It is not*, as misrepresented by critics, *a method* of doing C/H linguistics and therefore it is also not a replacement for standard methds. The key to utilizing the test is recognizing that *every language has its own set of phoneme frequencies* and therefore every pair of languages has a different set of paired phoneme frequencies. Thus, *comparison must be binary* until or unless someone develops the mathematics to do n-ary comparisons under these conditions. (But is doubtful that this latter is a desirable goal, since n-ary comparisons can always be decomposed into binary ones).

Ringe has applied the Test or reasoning derived from it at the 99% level to Indo-European (with a positive result), Indo-Euralic (very likely related but perhaps not reconstructable), Illich Svitych's "Nostratic" (negative), and Greenberg's "Amerind" (negative) and to other problems which he will discuss in his paper in this session.

2. African examples

Time constraints do not allow me to present detail in this section. I have presented the Omotic and Nilo-Saharan results elsewhere (Forth. and 1994 unpub. respectively) and will present the East Sudanic results at the 6th Nilo-Saharan Conference. The latter will be a study of what the Ringe Test reveals at 94% level for the nine proposed branches of East Sudanic with a detailed analysis of how the results compare with MLD and C/H results for the same units.

3. Greenberg's African Classification

3.1 Background

Greenberg's genetic classification of African languages was a major breakthrough in a field which was somewhat chaotic. It was first a series of articles (Greenberg 1949-54 in *Southwestern Journal of Anthropology*, v. full references in G1963), then collected in a small volume (1955). The final report (1963) was reprinted unchanged in 1966 and 1970, incorrectly referred to as "2nd and 3rd editions". It was at first largely rejected, especially in Europe, for mainly invalid reasons: rejection of genetic classificaton as such, non-acceptance of applying the method to "exotic" languages, reluctance to abandon preconceptions such as the racist "Hamitic" concept or the idea that Bantu is an archaic family.

While giving Greenberg full credit for his accomplishment, one should not over-emphasize the degree of chaos pevailing in the African classification field as of 1949 when his articles began to

appear nor the originality of his classification. Much of the groundwork had been laid by predecessors such as Westermann and Köhler, whom Greenberg properly acknowledges (e.g. see Ruhlen's survey history in his 1987: 76-124).

Many of the problems were relatively small-scale, e.g. the position of Fula and Hausa, both having the "isolated-language" mystique at the time, the position of Bantu, which some thought must be a major unit since it is so widespread and has so many varieties. Others were the hangover of the racist "Hamitic"concept, which wrongly brought physical type and cultural traits such as pastoralism into linguistic classification and needlessly complicated the placing of such languages as Masai. Others were backward concepts such as indiscriminate mixing of languages in "semi-" and "-oid" types, undocumented massive borrowings, and merging of typological and genetic classification. Others were the more prosaic lack of data in Chadic other than Hausa, southwest Ethiopia, and many other areas.

Greenberg's accomplishment was to arrive at the first continent-wide genetic classification based on sound-meaning coresponddences and cutting through the mass of misconceptions prevalent at the time. Its positive points far outweighed its shortcomings.

3.2 Criticisms

But there were and are legitimate criticisms of the Greenberg classification. I will refer herein mainly to Nilo-Saharan, which was and is the most controversial unit.

(1) Errors. Many which appeared in the 1955 collection were maintained in the 1963 revision. For example (as pointed out by Winston 1966), the little table of forms (1955: 109) which was to serve as an example of the method later known as Multilateral Comparison (MLC) contains several egregious errors. As Winston indicates and I amplify, this does not inspire confidence in the method or its application. The most serious error is listing under the gloss 'hand' items in Bantu and Kanuri which mean 'head' (also I now find in Teda, Zagawa, and Berti). Worse yet, the corresponding Efik term really means 'father' and is compared positively to the Bantu term. The table was repeated with the same errors in 1963 (p. 4, on which the method is referred to as "mass comparison").

Two other instances will suffice for now. On p. 111 (line 3 from bottom) of 1963 (not found in the 1955 version), a crucial argument is reversed with the wording: "7. Third person subject k- independent constructions." Context shows that what was meant is "...k- in dependent...". In the word list for East Sudanic, I counted only 7 attestatons of family 6 Temein out of 131 items (more on this below) and then found that one of them (no. 54 on p. 100) is really family 5 Nyima, mislabeled "(6) Nyima".

(2) Uneven documentation. Using East Sudanic (E.S.) as an example, there are in Greenberg's formulation 10 families: 1. Nubian, 2. Surmic (his Murle, etc.), 3. Nera (his Barea), 4. Jebel (his Ingassana), 5. Nyima, 6. Temein, 7. Tama (his Merarit, etc.),

8. Daju (his Dagu, etc.), 9. Nilotic, 10. Kuliak (his Nyangiya). The 131 lexical items (some with sub-varieties) include about 400 citations by families. Of these, half -92 Nubian and 109 Nilotic- are accounted for by two families. Temein has only 6 (with the erroneous no. 54 corrected) and Nyangiya only 10.

The others are E2 (abbreviation for Family 2): 45, E3: 37, E4: 21, E5: 17, E7: 44, E8: 19. None equals as much as half the citations of Nubian or Nilotic, which are by far the best-attested families in E.S. In fact, the Nubian and Nilotic citations are so high partly because items are cited which occur in single languages or sub-families of these entities. Using "common Nubian or Nilotic" or better yet reconstructed forms (which Greenberg rejects in his methodology) would largely redress the imbalance.

The same remarks apply to the morphological elements given in support of East Sudanic, but I will not go into this here.

Another aspect of this problem is the fact that the "mass comparison" is based on mainly 2, 3, or 4 families out of the possible 10 in E.S. In fact, the numbers of families involved are as follows: two: 52, three: 46, four: 24, five: 5, five: six: 2, seven: 1, eight: 1 (total 131). Thus, double and triple instances account for most of the evidence and only nine cases out of 131 include more than four families out of the ten. I will return to this below.

For N-S, the imbalance is not so marked: as expected, the vast Chari-Nile group is over-represented and the single language For is under-represented. However, the main problem here is the skewing introduced by the very presence of the invalid "Chari- Nile" grouping.

(3) Data do not support results. As Goodman (1970) and Bender (1976) show, Greenberg's' "Chari-Nile Family" consisting of East Sudanic, Central Sudanic, Berta, and Kunama is not justified by the data. I found East Sudanic itself to be coherent, whereas Goodman did not. This is the main error; others are minor, given the status of the comparative data available at the time.

3.3 Success despite faulty methodology and application

Others have pointed out similar problems with Greenberg's data and analysis of the other phyla (e.g. Leslau 1958 for Afrasian etymologies). Nevertheless, Greenberg's classification has won out and forms the basis for most Africanist work today. In my view, his Nilo-Saharan was a brilliant accomplishment. My own intensive work of two decades leads me to the following revision, based on morphological innovations (Bender 1989, 1991). It is best seen in diagram form (next page). Changes from Greenberg are given immediately following the chart.

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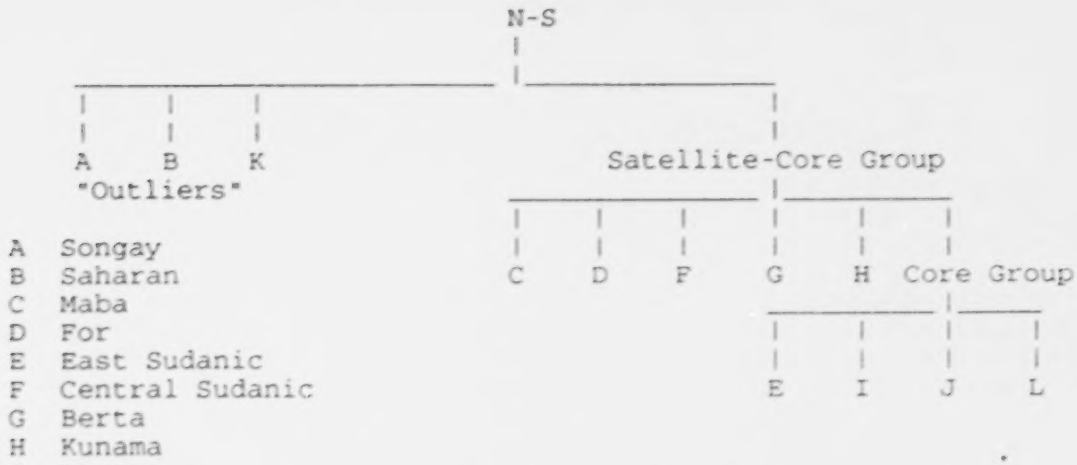

A Songay
B Saharan
C Maba
D For
E East Sudanic
F Central Sudanic
G Berta
H Kunama
I Koman
J Gumuz
K Kuliak
L Kado

Note that "Outliers" and "Satellites" do not constitute families.

Changes from Greenberg 1963:

Place Songay (Songhai), Saharan, and Kuliak (Nyangiya) as top branches coordinate with a large group called "Satellite-Core".

"Chari-Nile" is broken up: East Sudanic goes into the Core Group and the others (Central Sudanic, Berta, Kunama) are Satellites, coordinate to the Core and to each other).

Maba and For (Fur) are also Satellites.

Combine East Sudanic, Koman, Gumuz, and Kado into a Core Group. Kado is a new branch: it is Greenberg's "Tumtum" (1963: 149), which he stated diverged considerably from the other Kordofanian (of "Niger-Kordofanian") languages he grouped it with.

Within East Sudanic: delete E10 "Nyangiya" (Greenberg 1963: 128 note 3 expressed a reservation about Nyangiya belonging to E.S.); divide into two sub-families consisting of Ek: E1,3,5,7 and En: E2,4,6,8,9 respectively; these are based on retention of *k* in 1st-person pronoun in Ek and innovatioon of *n* in En (though E5 and E6 remain problematical) in their placements).

(I am omitting minor changes which apply to small groups or individual languages).

There are two main reasons I feel that Greenberg got such good results despite the flawed data base and analysis:

(1) He made use of some morphological innovations of the kind which appeal to orthodox comparative/historical (CH) linguisticians. To cite only a very few examples: the N/S pronoun pattern 1st/2nd/3rd person *a/i/e* and (more often in possessives) *a/o/e;* sg. N/pl. K (already suggested by Bryan- v. Tucker and Bryan 1966; but I think their distribution is more limited than they or Greenberg believed); verbal causative in *-t-*.

(2) More importantly, he identified a number of E.S. and N-S isoglosses in his "mass comparison" lists. I will consider the nine instances of Greenberg's most widespread items in E.S. mentioned in 3.2 above and then nine found in five or all six of Greenberg's proposed divisions of N-S.

3.3.1 East Sudanic

Referring to Greenberg's East Sudanic list (1963: 95-108), in citations I give representatives of the various forms he lists (he does not use *-forms or formulas), sometimes with capital letters to represent variatons (e.g. *K* means *g, k,* possibly *ŋ).*

G#32 'cow' in 8 groups (E1,2,4,5,7,8,9,10) as *t-* or *d-* often with *-N* or *-K.* G also has this under his N-S #41 along with C Maba. My E.S. isogloss **tei/taŋ.* (Here, the slash divides Ek from En, my two E.S. sub-families containing E1,3,5,7 and E2,4,6,8,9 respectively). Of course this term may be more likely evidence of cattle-culture diffusion in East Africa rather than genetic relationship (v. Bender 1982).

G#78 'mouth' (E1,3,4,5,7,8,10) in 7 groups as *aK, ŋal, kul, aulo.* Included partly in my Ek isogloss **aŋgul* and in N-S "Fragment" (not widely-enough distributed to be an isogloss) 'tongue' *NaL-* in families E and H.

G#86 'rain' (also 'river, water, sky') in 6 groups (1,5,7,8,9, 10) as *ar, korei,* etc. (also included in Greenberg's N-S set as part of his #109). My "excellent" N-S isogloss #2 **ar,* found in 10 families: ABK|CDFcGH|EI. (Here, the | separates "Outliers" from "Satellite-Core" from "Core" in that order; Fc is a family and is one of two sub-divisons of Central Sudanic, the other being Fp, not a genetic group in itself).

G#126 'who?' in 6 groups (1,2,3,5,7,8) as *na ~ ŋa* (and under N-S as #152 *ŋ(i)a).* Not included in my lexical comparisons, but in grammatical study (1991: 12) found as *-ŋ-* in CFG|E; thus it would be a possible isogloss for Satellite-Core (S-C).

G#4 'arrive' (also 'come') in 5 groups (E1,2,3,5,9) as *TVR* (also included in G's N-S as part of his #8; his inclusion of Gumuz *tona* is not justified). My "fair" N-S isogloss #164 *tOr+,* found in B|FpH|EkL.

G#59 'hand' in 5 groups (E1,2,3,5,8) as *ad, ed.* My Ek isogloss **at* (with citations also in En).

G#61 'head' in 5 groups (E1,3,4,7,9) as *ur, ol, kele.* My E.S. isogloss **Ur/Ol.*

G#65 'house' (also 'here, there, place') in 5 groups (E1,4,6,9, 10) as *ka, kwi, wee, oik.* Also found as part of G's N-S #78. I do not accept this one on phonological and semantic grounds, having only a Fragment in N-S of form *wai* in C Maba and an isogloss **wVl* in Ek.

G#117 'tooth' in 5 groups (E1,2,3,4,10) as *ni(gi)T.* My E.S. isogloss **ŋi+T.*

To summarize, it is clear that Greenberg's findings for these nine items are a good start, taking into account the data available at the time and the fact that N-S was only part of a much

wider project. The result certainly fulfills the pre-theoretical task of indicating East Sudanic as a probable family to be investigated rigorously. See Ross 1991 and Bender et al. Forth.

More generally, perhaps this result implies that MLC can work fairly well as a pre-test for families of moderate depth such as IE and E.S. (see 2, 3 above): eight of G's nine most extensive comparison sets coincide fairly well with cognate sets. But Nilo-Saharan is a greater challenge as we shall see.

3.3.2 Nilo-Saharan

Greenberg's N-S items (1963: 133-48) suffer from the unfortunate aberration of "Chari-Nile". His N-S consists of six families: Songay, Saharan, Maba, Fur, "Chari-Nile", Coman. The last-named combines my Koman and Gumuz (plus "Mao"languages, which are now known to be Omotic- see Bender 1989b). The numbers of families involved in G's 161 items are: two: 61, three: 68, four: 23, five: 7, six: 2. The nine best cases (5 or 6 families included) are:

G#81 'kill, die' in all six families as *wi, wu, yeyi,* etc. I have a Fragment (probably should be a weak isogloss in S-C) *wi, iy,* etc. found in A|CDF|I.

G#87 'lightning' in all six families as *mVl, mud-, bil.* I do not accept this one, having only Fragment *bEL* in K|E.

G#22 'blood, red' in all but D For as *(K)eri, KVR.* My "good" isogloss #40 **k+ar+* in AB|CFH|E and an overlapping item #323 of form **(k)ORi* in AB|CFpG|EL and also in Mande (as *Rali, Roli)* and possibly in Proto-Niger-Kongo as *Rodi, Roli,* in particular in Volta-Congo (see Williamson 1989 for classification) as *kre, kila.*

G#26 'breast, chest' in all but D For as *gani, akun,* etc. My "good" isogloss #45 **kin+t ~ kun+t* found in BK|CFH|EJ.

G#61 'fire' in all but A Songay as *azza, su, udu, ito, woti,* etc. This item is divided between my "fair" isogloss #159 **-SI* in B|CF|IL and Fragment *wut, ɔd,* etc. in D|I.

G#65 'go, walk' in all but D For as *KV.* This is divided between my "good" isogloss #34 **ga(w)o* in ABK|F|I and #315 **ka* in AB|CH|I, also found in Mn and Volta-Congo as *ka, kɔ.*

G#88 'lion, leopard' in all but I "Coman" as *mVr, muddu.* I have this as a "symbolic" item #270 **mEr* in ABK|CDGH|-, based on the possibility of feline sound-symbolism, although I myself find this unlikely. (I also have a possibly symbolic S-C isogloss #272 **ɲ-a(u)+* for the same meanings).

G#95 'mother' in all but B Songay as *ya.* My #278 **ya* in BK|CDFH|EJL, is considered to be a symbolic (nursery) term, again without much conviction.

G#109 'rain' in all but C Maba as *hari, war, koro.* This is the extension of Greenberg's E.S. #86 and is my #2 as above under E.S.

3.3.3 Conclusions

The Nilo-Saharan set of Greenberg's is more problematical than the East Sudanic one, but still includes enough of substance to make his Nilo-Saharan worth pursuing.

In fact, this is just what I have been doing. The degree of overlap of our results is impressive, especially when one considers that I have not consulted Greenberg's particular proposed E.S. and N-S forms in any extensive or systematic way in all the years of my work. None of the above items from Greenberg looked familiar to me when I tracked them down for this paper.

However, by relying on "mass comparison" and scorning regularity and reconstruction, Greenberg committed many errors. Examples are his using his E.S. #4, 32, 65, 86, 126 as evidence for both E.S. and N-S and doubtful judgments involved in the makeup of his E.S. #65 and 78 and N-S #61 and 87.

The "mass comparisons" missed most of my 16 "excellent", 69 "good" and 88 "fair" N-S isoglosses. Consider only the "excellent" ones: those which include representation in all four branches of N-S: A Songay, B: Saharan, K: Kuliak, and S-C Satellite-Core (see diagram in 4.3. above). (I have been very conservative in rejecting other potential "excellent isoglosses" because of possible symbolism or diffusion). I give these with only main glosses.

#1 'belly, intestines' **ar* in ABK|CH|EkJ
#2 'rain, river' **ar* in ABK|CDFcGH|EI
#3 'work, make, change' **bEr* in ABK|CH|IJL
#4 'stick, spear, bow' **bEr* in ABK|CDFG|EIL
#5 'wing, neck' **bi ~ bo* in ABK|CG|EnIJ
#6 'many, big' **bo* in ABK|FH|EnIJ
#7 'ashes, earth' **bo/an* in ABK|CH|EJ
#8 'rib, side, horn' **der* in ABK|DGH|-
#9 'brother, man' **er* in ABK|CD|EIL
#10 'follow, hunt' **kor* in ABK|CH|IJ
#11 'elbow, foot, finger' **kor*$_2$ in ABK|CDGH|EIL
#12 'horn, bone, rib' **k+Ob* in ABK|CDFp|EnIL
#13 'lake, river, well' **kuR* in ABK|CDF|EL
#14 'say, ask, count' **nV* in ABK|CDFG|EJL
#15 'many, all' **Pat* in ABK|CDGH|L
#16 'fall, return' **tI+t* in ABK|CDFH|IJL

Of these, only #2 appeared in the discussion of 3.3.1-2 above (as G#86 under E.S. and G#109 under N-S). It is very revealing that very few of the above isoglosses are reflected in Greenberg's three lists of "mass comparison" items: East Sudanic (1963: 95-108), "Chari-Nile" (ibid. 117-127), Nilo-Saharan (ibid. 133-148). These are pieces of my #4 in "Chari-Nile" #90 (Nilotic and Kunama), pieces of my #5 in C-N#3 and N-S #5 (Saharan, Maba, Berta, Didinga, Koman), pieces of my #7 in C-N#9 and N-S#9 (Songay, Berta?), pieces of my #9 in E.S.#71 and N-S#91 (Songay, Saharan, Nubian, and Nilotic). There are single instances elsewhere also.

The conclusion that the "method of resemblances" (Heine 1972)

or "mass comparison" or "multilateral comparisons" is doomed to failure as other than a pre-theoretical first step: it results in missing most of the good candidates for isoglosses by jumbling together parts of real isoglosses and items wrongly judged to be cognates on the similarity basis. For one example of the latter, G's N-S#9 includes my #7 and also my #336 *bUr*, not an N-S isogloss, being found widespread also in Afrasian and in Mande.

Another interesting way in which MLC misses the boat is that it fails to reveal such interesting phenomena as that illustrated in item #11 above, namely the regular correspondence sets with patterning according to sub-classification. In #11, r_2 stands for a proposed proto-phoneme which is realized as *r/r ~l/l* in the modern languages (recall that the notation means Outliers/Satelites/Core). My analysis revealed also similar d_2, t_2, and e_2 (see also o_2 or *o/a* in #7 above).

To conclude, Greenberg's Nilo-Saharan work succeeded not because of but despite his espousal of "Multilateral Comparison". It included a large-enough data base and enough sound judgments to lead him to the right outline of N-S despite his rejection of regular correspondences and reconstruction and the slipshod appearence of much of the supporting presentation. I believe this conclusion would also apply to the Niger-Congo (=Niger-Kordofanian) and the Afrasian (=Afroasiatic) work. I reserve judgment on the Khoisan work because I have no expertise in that area. This is another illustration of what Newman (1974: 648) refers to as being able to recommend the cook but not the cookbook!

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Notes

1 Note that this is not exactly parallel to tossing a coin or rolling dice. We cannot assume that there are a fixed number of forms which is the same at each trial like the six possibilities for one die. For each language, the forms are largely different from each other language (if not, there would be little point in comparing them) and we are looking for how many times forms from one language are similar enough to that in others to be considered a "match". This is like looking for how many 1's, or 2's, etc. are rolled in 12 trials with a die except that of course all 1's are not similar but actually identical. See the attached chart for how this works for the first two trials.

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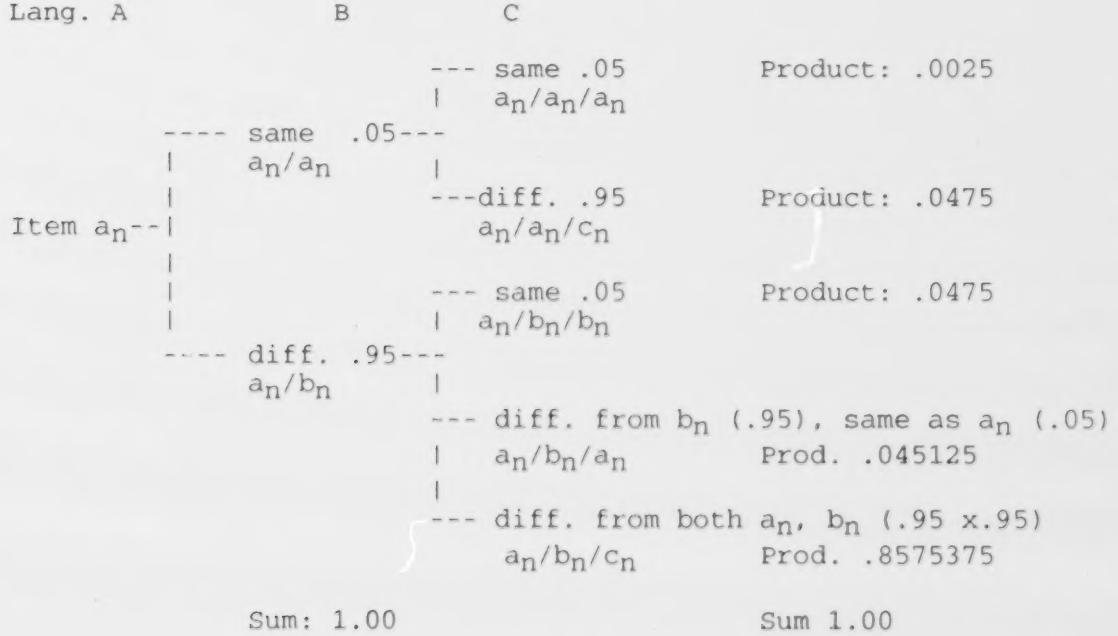
Handout for M. L. Bender: Testing Multilateral Comparisons in Africa (LSA Meeting Jan. 5, 1995)

Assume 12 languages abbreviated as A, B, C,...L; 100-item lists for each, excluding loans, compounds, etc. Examine item n on the list in A, B, etc. in turn. Call the form for item n in language A a_n, etc.

Assume that the MLC "measure of similarity" from language to language is a modest 5% (Greenberg 1963: 3 refers to it as "accident [sic] resemblances between two languages" and in his example uses 20% to make his case stronger). It is clear from context that he means this to apply to any two languages under consideration).

Now develop the branching tree from left to right for (judged to be) "same" (5% probability) or "different" (95% probability). We run into a problem after the second language: the third item may be judged "same" or "different" as either the first (in language A) or second one (in language B). This is unlike throwing a die for which the outcome is simply the number which turns up.

To develop the tree we have to assume *transitiivity for "same":* if a is "same as" b and b is "same as" c, then a is "same as" c. But this does not hold for "different from" since a can be "diff. from" b and b "diff. from" c, but a and c can be "same"!


Lang. A
B
C
Item a_n
same .05 a_n/a_n
diff. .95 a_n/b_n
--- same .05 $a_n/a_n/a_n$ Product: .0025
---diff. .95 $a_n/a_n/c_n$ Product: .0475
--- same .05 $a_n/b_n/b_n$ Product: .0475
--- diff. from b_n (.95), same as a_n (.05) $a_n/b_n/a_n$ Prod. .045125
--- diff. from both a_n, b_n (.95 x.95) $a_n/b_n/c_n$ Prod. .8575375
Sum: 1.00
Sum 1.00

The two extremes of the chart are simple. Greenberg's case is the top: probability of all forms agreeing. The bottom is the case I discuss on p. 2: all items are different. *Everything except the bottom* is the case of *at least one* match. The problem arises with the middle of the chart where there is *exactly* one match. Of course the middle gets more and more complicated as the comparisons are extended to four, five, etc.languages and there can be exactly one, two, three, etc. matches.